has been fully discussed elsewhere without my knowledge of it. Is the fact that the sting of the work r-bee is an imperfect weapon of defence, a result of its having nothing to do with the propagation of its species, this being left to the stingless queen and drones? Consequently any tendency to develop a more effective sting in one generation of worker-bees has no hereditary effect on succeeding generations, nor apparently have the worker-bees any influence whatever on the worker-bees that succeed them, except by the way in which they feed and educate them, unless, indeed, they can impress their tendencies on the drones or on the future queen before she leaves the hive. If they have no such power, it seems likely that they will always have to lament the use of a weapon which nature might have made as effective as the sting of a wasp. Finally, are there any other insects in the same predicament as worker-bees, *i.e.*, unable to use their weapons of defence without doing themselves more injury than they inflict on their adversary, and unable to help their successors by the transmission of a continually accumulating instinct? R. A.

Manningtree, January 22

Molecular Vibrations

MR. CHAPPELL is certainly right in stating that "the noises in a belfry are most discordant." He might have said (what no doubt he meant) that the sounds emitted by each single bell are most discordant. Every bell which is at all tolerable, possesses, it is true, one predominating note due to the thick part of the bow, where the clapper strikes, but there are also innumerable other notes, some of which *may* be harmonics, while the majority are not so at all. This is presumably often owing to flaws and other defects in casting, but there is another cause common to every case, which is due to the following fact :—

All bells are cast of a conventional shape, with varying diameters from bow to crown. Now every part of a bell, taken vertically, comes into vibration when struck, and in order to give a true note, each horizontal section ought to have a certain exact thickness of metal proportional to its diameter. This is easily verified to the ear by tapping the bell gently at all parts from the bow upwards. Every inch gives a different rate of vibration, and, consequently, a different pitch.

About the time when the second "Big Ben" was cast, which is a long time ago, I tried experimentally to ascertain what the law was which regulated the thickness of the metal in relation to the diameter of the bell, so that every section might be of identical pitch. This was done by casting a series of bell metal rings of varying diameters, and tuning them, by turning in a lathe, to exact unison. So far as my recollection now serves me, the following was the result :—

Measuring all the rings by their outside diameters, no undeviating rule was apparent, and the same was the case when the inside diameters were compared. When, however, a circle was taken whose circumference was, as nearly as possible, one-third from the outside of the thickness and two-thirds from the inside, then the law came out distinctly that the thickness of the metal must be proportionate to the square of the diameter of such circle. It occurred at once that this circle must, in fact, constitute the neutral axis of vibration. Working on this principle, it seemed worth while to try whether a bell could not be constructed free from discordant sounds. I may shortly say that this proved to be possible, but only by turning the actual casting with great care and accuracy in a lathe. It became evident that the slightest variation in the true thickness vitiated the unisonal character of the tone. A "miss was as good (or as bad) as a mile," and consequently the process of casting itself was too rough for obtaining the desired end.

It may fairly be gathered from Mr. Chappell's letter that he is not enamoured of a "triple bob major," and that he does not class bells generally as musical instruments. I am much afraid he never will. If the present shape and mode of construction (and let me add, the present mode of change ringing) is adhered to, a peal of bells which will quite satisfy a musical ear may be regarded as a practical impossibility. R. H.

Missing Nebulæ

IN the note on missing nebulæ in NATURE, vol. xix. p. 221, I find the nebulæ G. C. 132, 4570, and 5051 mentioned together with the Merope nebula as being diffused objects which are "overlooked in very large telescopes, though obvious in much smaller ones." This alludes, no doubt, to the occurrence of these objects in the list of nebulæ not found with Lord Rosse's 6-foot reflector (*Phil. Trans.*, 1861, p. 745).

With regard to the first object, G. C. 132, it has only been looked for once at Birr Castle, and in the N.P.D. 111° 30′ it is possible to account for its non-appearance either by a tilting of the speculum or by the haziness of the sky in this low altitude. G. C. 4570 has been seen three times, and only twice searched for in vain, both times in twilight. G. C. 5051 was set for twice and not found, but 15° north of the zenith the tilting of the speculum almost always changes the index-error of the setting-circle considerably, as expressly stated by the observer on one of the two occasions alluded to. The Merope nebula was last winter seen very distinctly, and roughly sketched with a low power and large field. J. L. E. DREYER

The Observatory, Dunsink, Co. Dublin, January 13

Time and Longitude

NOW that mankind begin to have settlements, even continental, as appears from Mr. Latimer Clarke's account of Sitka, subject to the inconvenience that he and Mr. Layard point out, is it not time that we agreed to make the line dividing "yesterday from to-morrow" avoid all continents, by taking advantage of two very convenient, if not providential, facts, which are certain, though each was *à priori* highly improbable? First, there were great chances against a globe with our existing proportion of land to water, of coast-lines to area, and of large and small lands to each other, having any Behring Strait, admitting one degree of longitude, or thereabouts, to enjoy the above property. But next, there was still greater chance, perhaps, against the exact opposite degree to the strait covering several national observatories; not only more of them, I think, than any equally narrow meridional band, but the only one that, on historical grounds, we can conceive distant civilised nations accepting without jealousy as a common centre. The antimeridians of Copenhagen, Uraniburg, Leipzic, Munich, Padua, Venice, and Florence, seem to avoid both continents; possibly also those of Christiania, Gotha, Verona, and Modena. Those of Berlin, Prague, Naples, and Palermo, seem a very few miles too far east. Europe proper, and its present railways, are very closely bisected by this street of observatories; the local time of the furthest points each way varying but an hour and a half from it. But the chief coincidence is yet unnamed. Would the pride of any existing land, except China, refuse to make a standard meridian of Rome?

The very Chinese must allow Europe a sort of scientific precedence, not as the metropolitan, but the learned continent—earth's university. Europe alone is the adult continent, if there be one; and no other has in a strict sense a metropolis. The history of no other has so turned upon one pivot city as that of Europe has on Rome, nor is likely ever to do so. Some one says that "what a church is to a city, Palestine is (or may some day be) to the world;" but it is less disputable that what the market-place is to a city, Europe is to the world—perhaps permanently. And what the tribunal is to the market-place, Rome has been to Europe, as long as Europe was growing. Observe, too, that in this special connection both our civilised time-reckonings, "Old Style" and New, have come from Rome. Might we not also supersede the distinction of E. and W. longitude, by calling Rome 180°, and reckoning all round, from Behring to Behring, leaving the 0° as yet unmarked? E. L. G.

[E. L. G.'s proposal has been already made by M. de Beaumont. See NATURE, vol. xix. p. 247.—ED.]

Shakespeare's Colour Names

I FEAR you will think that the correspondence on this subjec is becoming a mere criticism on Shakespeare's text, and therefore out of place in your columns, but I trust you will afford me space for a short rejoinder to Mr. Ingleby's letter (NATURE, vol. xix. p. 244).

I am obliged to him for pointing out that Sir T. Hanmer had already suggested the substitution of "keen" for "green" in the passage from "Romeo and Juliet," Act iii. Sc. 5. This had escaped me, but I cannot agree with him that the alteration has been *rightly* rejected by subsequent commentators. I have not at present any opportunity of examining the eyes of any living eagles, but in opposition to Mr. Craig-Christie's evidence (NATURE, vol. xix. p. 221) I must point out that all our best

British ornithologists—Yarrell, Macgillivray, Gould, Meyer, and Morris—describe the eye of the Golden Eagle (the less rare of our two British species, and the one usually referred to by our poets) as *hazel* or *brown*. The eye of the Sea Eagle is described by the same authorities as yellow. I cannot think that so accurate an observer of nature as Shakespeare would call either hazel or yellow eyes *green*. Can Mr. Ingleby cite any authority for such a comparison as "green as is an eagle's eye"? while the keen piercing sight of the bird is as proverbial as the swiftness of its flight. I am well aware that green eyes were held in high estimation by the old poets, especially by those of Spain; Shakespeare, however, does not seem to me to have shared in this predilection, as, setting aside the doubtful play of "The Two Noble Kinsmen," and the passage now in question, he uses the epithet three times only, I think, as applied to the eye, and then always *in malam partem*, viz., "green-eyed jealousy," "Merchant of Venice," Act iii. Sc. 2; "It is the green-eyed monster," "Othello," Act iii. Sc. 3; and in "Midsummer Night's Dream," Act v. Sc. 2, where the "eyes as green as leeks" are met with in conjunction with "lily lips," "cherry nose," and "yellow cowslip cheeks." I cannot think with Mr. Murphy (NATURE, vol. xix. p. 197), that the eyes which the old poets so admired as green were what we call blue; they were more probably *grey*, which often has a shade of green in it—the "eyen grey as glas" of Chaucer's "Prioresse." These green or grey eyes were, I think, usually an attribute of feminine rather than masculine beauty, as in the passage from "The Two Noble Kinsmen," Act v. Sc. 1, where they are mentioned in an address to Diana (not Neptune, as Mr. Ingleby has it). Shakespeare well distinguished between the different colours of eyes—see "Two Gentlemen of Verona," Act iv. Sc. 4, and "Twelfth Night," Act i. Sc. 5, for grey eyes; "As You Like it," Act iii. Sc. 2 for blue eyes; "Romeo and Juliet," Act ii. Sc. 4 for black and grey eyes, and Act iii. Sc. i. of the same play, where hazel eyes are mentioned.

ROBERT BREWIN

Exeter, January 20

Intellect in Brutes

SIR HARRY LUMSDEN allows me to publish the following little incident:—Late last autumn some partridges, which he had tamed and kept about the house, disappeared as usual and became wild. When the excessive cold set in and Aberdeenshire was deep in snow, Sir H. Lumsden was greatly pleased and surprised one morning to find his old friends on the doorstep waiting to be fed. Next morning they appeared with a wild covey of eleven birds, and the tame cock sat on the doorstep and crowed to the wild birds, evidently encouraging them to come and eat the food, which, however, they declined to do till it was put further from the house. Soon after the tame birds appeared with two covies. How did they entice the wild birds except by actual bird talk?

WALTER SEVERN

Feeding a Python

THE attack of a constrictor, at all events in confinement, is very often unsuccessful; but perhaps this may be because the reptile is not hungry. I have often seen the constrictors in the London Zoological Gardens strike several times at birds, pulling out feathers and even getting a firm hold and then releasing their prey, to renew the attack presently either with or without success. When the membrane over the eye is becoming opaque in consequence of the change of skin they frequently fail to hit the prey at all, but still persist until they secure it. I saw one of the large pythons take a rabbit in a way which must be unusual, I think. The rabbit was hopping about near the snake's coils when the reptile suddenly made a loop in its body, and firmly inclosed the victim without touching it at all with the mouth, or even raising its head. The rabbit died there, but the snake paid no attention to it for a quarter of an hour and subsequently swallowed it very leisurely.

ARTHUR NICOLS

THE GRAHAM LECTURE, ON MOLECULAR MOBILITY

THIS lecture, the institution of which was referred to in NATURE, vol. xix. p. 254, was delivered on the 22nd inst., by Mr. W. Chandler Roberts, F.R.S., Chemist of the Mint, before the Philosophical Society of Glasgow, in the hall of the University, where Graham graduated in 1824.

The audience, which was very large, included most of the professors of the University.

Mr. James Mactear, president of the Chemical Section, pointed out that they were doubly fortunate in having secured the services of Mr. Roberts, whose co-operation in his work Graham repeatedly acknowledged in the warmest terms, and in the fact that Mr. James Young, F.R.S., of Kelly, the life-long friend of Graham, had consented to preside on this occasion; he therefore vacated the chair in favour of Mr. Young, who introduced the lecturer.

Mr. Roberts briefly traced the influence of Black and Thomson in turning the attention of Graham to the study of molecular physics, to which he patiently devoted his life. In connection with the law of the diffusion of gases the lecturer claimed that Priestley made in 1799 an observation on the escape of hydrogen from a cracked jar. The subsequent and independent discovery of this phenomenon by Doebereiner in 1823 has hitherto been considered the starting-point of the experimental study of gaseous diffusion to which it undoubtedly attracted Graham's attention. After a brief review of the influence of Eastern and Greek thought on the study of molecular movement, allusion was made to Sir Christopher Wren's model representing the effects of all sorts of impulses that result from the impact of hard globulous bodies, which, according to Dr. Sprat, historian of the Royal Society, he proposed as the principles of all demonstrations in natural philosophy, it being considered "that generation, corruption, and all the vicissitudes of nature are nothing else but the effects arising from the meeting of little bodies, of different figures, magnitudes, and velocities."

Herepath's revival of Bernoulli's view as to the movement of gaseous particles was considered, and Mr. Roberts then described in detail the experiments that enabled Graham to establish the law of the diffusion of gases, and he illustrated experimentally the passages of gases through porous bodies, such as unglazed earthenware and artificial graphite, as well as through a layer of the hard translucent variety of opal known as hydrophane. The mode in which Graham studied the diffusion of the momentum of gases, by observations on viscosity as indicated by rates of flow through capillary tubes, was then described. It was pointed out that his law of diffusion forms the basis of the science of molecular mechanics, and his measurements of the rates of diffusion prove to be the measure of molecular velocities which have been so profoundly investigated mathematically by Clerk-Maxwell, Clausius, and Boltzmann, and experimentally by Loschmidt in developing the dynamical theory of gases. The lecturer then considered the passage of gases through colloid or jelly-like bodies which have no sensible pores, dwelling more especially on the separation of oxygen from air by the transmission of air through a thin film of india-rubber, a circumstance of special interest from a physiological point of view.

The liquefaction of gases formed the subject of one of Graham's earliest papers, in 1826, and it occupied his attention at intervals during his life. He held the view that hydrogen when absorbed by palladium is reduced to the metallic form, a supposition which has received strong confirmation from the success that has attended M. Raoul Pictet's efforts to solidify this gas; and that distinguished physicist stated in a letter to Mr. Roberts that it is probable Graham's indication of the density of solid hydrogen will prove to be nearly correct. Allusion was then made to Graham's opinion that the various kinds of matter now recognised as different elementary substances may possess one and the same ultimate or atomic molecule existing in different conditions of movement, the varying degrees of rapidity of this movement constituting, in fact, the difference between the elementary